International Conferences on Recent Advances in Geotechnical Earthquake Engineering and Soil Dynamics

1991 - Second International Conference on Recent Advances in Geotechnical Earthquake Engineering & Soil Dynamics

12 Mar 1991, 10:30 am - 12:00 pm

Session 1: Discussions and Replies

Multiple Authors

Discussion on paper titled: "Modelling the Deformation of Sand during Cyclic Rotation of Principal Stress Directions," by Marte Gutierrez, Kenji Ishihara and Ikuo Towhata, Paper No. 1.3

By: Anil Misra, Assistant Professor of Civil Engineering, University of Missouri, 600 W Mechanic, Independence, Missouri

The paper describes an elastoplastic constitutive model for predicting the stress-strain response of sands subjected to a continuous rotation of principal stress directions. The focus is on modelling sand behavior for loading conditions under which the intermediate principal stress remains constant, such as the conventional triaxial test and b=constant tests ($b=(\sigma_2-\sigma_3)/(\sigma_1-\sigma_3)$). The yield and potential surfaces are defined in a p-X-Y stress space which may be appropriate for the above loading conditions.

The proposed model is employed to predict the behavior of Toyura sand under continuous rotation of principal stress directions at constant b (=0.5) and constant stress ratio ($\sigma_1/\sigma_3 \approx 3$). The authors must be commended for obtaining excellent agreement between the model predictions and the measured results.

The success of the proposed model seems to critically hinge upon the validity of the flow rule and the yield criteria adopted in this model. The flow rule and the yield criteria therefore must be carefully assessed for a wider range of loading conditions.

For example, the flow rule indicates that a continuous rotation test conducted at the failure stress ratio, i.e. stress path along the failure surface in the X-Y plane, will predict the strain increment direction to be normal to the stress increment direction. This implies that if the sample were brought to the failure state along the deviatoric stress axis, i.e. X axis, and subsequently the principal stress direction were rotated by applying an incremental shear stress, the sample will experience normal strains at that instant. Such a model prediction needs to be experimentally verified.

Furthermore, the stress-dilatancy equation employed in the model renders the proposed stress-strain relationship non-symmetric. This has implications on the stability of the proposed constitutive law.

In any case, the authors must be congratulated for their successful attempt at modelling an extremely complicated facet of the deformation behavior of sands.

DISCUSSION by Predrag Kvasnička,

Faculty of Civil Engineering,

University of Zagreb, Yugoslavia on

"Dynamic Properties of Saturated Coal Fly Ash" by Pei-Ji Yu and Wei-qin Qin (PAPER No. 1.10)

The shear modulus and damping of fly ashes determined by resonant column tests are presented in the paper. Very interesting is the effect of aging on dynamic properties of fly ash.

Of special concern is fig. 8 (Cyclic strength ration vs. mean diameter of grain). The diagram shows that cyclic strength ratio is smaller for greater percent of fines what is the opposite trend of the data presented in Ishihara (1978), Townsend (1978) and Seed at al. (1984) (after Committee, 1985). The data presented by Seed et al.. is according to Chinese Code Proposal (for fines content grater than five percent) and shows greater cyclic strength ratio (resistance to liquefaction) for the greater fines content.

I am asking the authors to comment or give their explanation on the influençe of percent of fines on cyclic strength ratio.

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Discussion on paper titled: "Deformation of Sand Under Cyclic Simple Shear Loading," by Chung-Jung Lee, Paper No. 1.12

By: Anil Misra, Assistant Professor of Civil Engineering, University of Missouri, 600 W Mechanic, Independence, Missouri

A stress-dilatancy relationship is derived for sand deformation under simple shear loading. The author assumes that the deformation occurs along a shear plane. The derived relationship is in terms of the inter-particle friction angle and a fabric factor. Reasonably good agreement is obtained with the measured results and predictions based on the relationship.

It would be of interest to compare the derived relationship with the Rowe's dilatancy equation. Furthermore, the physical interpretation of the fabric factor needs to be clarified. Is the orientation of the shear plane a factor in the derivation and is it related to the fabric factor?

DISCUSSION by Predrag Kvasnička,

Faculty of Civil Engineering,

University of Zagreb, Yugoslavia on

"A Cyclic Shear-Volume Coupling and Pore Pressure Model for Sand" by Peter M. Byrne (PAPER No. 1.24)

In the paper, a single two-parameter incremental shear-volume coupling equation for sand has been presented.

The equation is based upon laboratory tests and gives the values that are in good agreement with laboratory data over a range of relative densities.

In the lack of laboratory data for particular sand, the author suggests their estimation by means of relative density or corrected number of blows of standard penetration test (SPT).

I personally, as a member of laboratory staff, am happy that the test, formulae, and predictions of liquefaction, based on suggested formulae are in so good agreement with well known data by H.B. Seed and others, because, to my knowledge, most of their results is based on in situ measurements. It seems to me that, for some time, the role of laboratory testing was a little bit neglected.

At the same time, I am aware that material data obtained in even perfectly ran laboratory tests are strongly influenced by sample disturbance. The question is: Is so good agreement with field results due to fact that normalised relations, such as that on figure 4., are less sensitive to sample disturbance, than stress-strain relations. In other words: is the shape of curve the same for soil element and for "undisturbed" sample?

In the other hand, the author suggests estimation of C_1 and C_2 via relative density (D_r) and/or normalized (SPT) results. As a matter of fact, relative density is obtained from minimum and maximum density which are dependent on test conditions. Another problem is in situ density of sand. Assuming all the disadvantages of SPT, one should be very careful in implementing formula (8) for estimating of D_r.

What is the correlation between C_1, C_2 and more confident in situ methods, such as cone penetration or dilatometer tests?

Then, for instance, for estimating G_{max}, the influence of overconsolidation and horizontal stresses could have been taken into account, and not only the influence of D_r and σ_m', as in equations (18) and (19).

The recommended value of threshold strain $_t$ seems too low, since Dobry et al. (1982) and Vučetić (1986) explained that theoretical value of $_t$ is about 10^{-2}% and "practical" values of $_t$ obtained from strain-controlled cyclic tests are of the order 2-5x10^{-2}%.

DISCUSSION ON

"APPLICATION OF THE ELASTOPLASTIC-VISCOPLASTIC BOUNDING SURFACE MODEL TO CYCLIC LOADING"
By Victor N. Kaliakin
(Paper No. 1.31)

By: Ronaldo I. Borja
Assistant Professor
Department of Civil Engineering
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A constitutive model based on the notion of bounding surface elasto-(visco-)plasticity is described in this paper. Although the specific form of the bounding surface used by the author is fairly standard, the introduction of irrecoverable deformation inside the bounding surface makes the model somewhat different from conventional plasticity theory of critical state soil mechanics.

The model may be useful in practice because it has the capability to replicate some of the most important features of soil behavior, particularly those associated with cyclic soil response. As for monotonic response, the model might also have the capability to replicate stress-relaxation, creep, and even quasipreconsolidation effects. (For example, take a zero stress-rate, $\dot{\sigma} = 0$, in Eq. (3b) of the paper; then the soil creeps due to the viscoplastic soil response according to Eq. (3a). Substituting the creep deformation in Eq. (9) then results in the growth of the preconsolidation pressure I_0 with time.) However, since the rate-dependent component of soil deformation is derived from a viscoplastic formulation, the accuracy of the model in reproducing typical long-term responses of soils at conditions of either sustained stresses (i.e., creep) or sustained strains (i.e., stress relaxation) might not be good.

The viscoplastic theory assumes that there exists a fixed state of stress to which the solution will tend (the inviscid solution), and that delayed deformations will eventually cease when the inviscid solution is reached (in the context of the present model, the inviscid solution could be any point on the surface of the elastic nucleus). Since continued creep deformation (or stress relaxation) is a characteristic feature of real soil behavior, the present theory may not be appropriate for modeling such a soil response.

This discusser notes with curiosity how the author tackled the problem of stress-point integration of the rate-constitutive equation. A stress-point integration algorithm is a crucial component of any rate-type constitutive model; in fact, it could either make or break a modern finite element code. A standard incrementation procedure based on the use of elasto-plastic soil moduli will surely make time-stepping difficult for the problem at hand since the specific form of the present model is based on critical-state theory (which is known to be specially sensitive to time-stepping). On the other hand, the powerful return-mapping algorithm used successfully in most computational plasticity problems may not apply directly to models of the bounding surface type since no consistency condition is imposed inside the bounding surface, and so no interior yield surface exists to which the stress predictor could be "returned" or "mapped". A discussion of the stress-point algorithm used by the author and its performance thus seems warranted.

Discussion on paper titled: "Downhole Seismic Cone Analysis Using Digital Signal Processing"
by: R.G. Campanella and W.P. Stewart
Paper No. 1.32
Discussion by Wanda Henke, Dynamic In Situ Geotechnical Testing, Inc.

In this paper the authors present practical aspects of the seismic cone penetration test (SCPT). The authors first discuss equipment required for the SCPT. The authors then discuss data analysis techniques used to determine shear wave velocity from SCPT data. The authors also discuss filtering techniques for processing data prior to analysis.

The information provided should be useful to practitioners and researchers using the SCPT or similar equipment and analysis techniques in other applications.

The authors make recommendations related to equipment and signal analysis techniques for optimum estimates of shear wave velocity. They discuss potential problems associated with the SCPT and the mitigation of these problems through the selection of proper equipment and signal analysis methods.

A discussion of the effects of different sources (different shapes, sizes, materials, hammers, etc.) and different soil types on the frequencies and amplitudes of shear waves being generated would be helpful since these would seem to significantly affect the selection of equipment.

Discussion on paper titled: "Insitu Measurement of Damping of Soils"
by: W.P. Stewart and R.G. Campanella
Paper No. 1.33
Discussion by Wanda Henke, Dynamic In Situ Geotechnical Testing, Inc.

In this paper the authors discuss the use of data obtained from seismic cone penetration tests (SCPT) to determine damping, at low strain levels ($10^{-4}\%$ - $10^{-3}\%$), of two soil layers, an upper sand layer and a lower clayey silt layer. Relevant theory and signal processing techniques are briefly discussed. Then specific methods and results are presented and discussed.

The authors compare three methods for determining damping from acceleration records obtained from SCPTs. They indicate that two methods, the attenuation coefficient method and the modified SHAKE method, required somewhat complicated corrections, gave scattered results, and gave negative damping for the lower clayey silt layer. They indicate that the third method, the spectral slope method, did not require corrections, gave less scatter, and did not give negative damping for the clayey silt layer.

It seems that at least some of the differences between results from the three methods can be explained by how they were applied. When using the attenuation coefficient and modified SHAKE methods, the authors determined damping profiles and inferred average values from these. In contrast, when using the spectral slope method, the authors did not determine damping profiles. I feel that the comparisons would have been more consistent and informative if a damping profile had been developed using the spectral slope method. Based on an inspection of Figure 13, I think that such a profile would have shown scatter; however, I feel the scatter would have been less than that resulting from use of the other two methods. Also, I feel the average damping of such a profile would likely have been positive for the lower clayey silt layer.

The determination of damping using the SCPT would seem to be far more difficult than the determination of shear wave velocity. It seems, from a physical standpoint, that to avoid large scatter highly consistent and accurate measures of peak accelerations in the soil would be needed, particularly for lower levels of damping. Obstacles to that might include repeatability of the source and soil-probe interaction.

DISCUSSION ON
"MECHANICAL PROPERTIES OF CEMENTED SANDS BASED ON INTER-PARTICLE CONTACT BEHAVIOR"
By Anil Misra and Ching S. Chang
(Paper No. 1.34)

By: Ronaldo I. Borja
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This paper provides a micromechanical interpretation of the macroscopic behavior of cemented sands. Details of the analytical treatment are described on the granular level, with an extensive use of the micro-macromechanical transformation equations formulated previously by Hill, among others (most of the references cited in the text, particularly the more important ones, are unfortunately not to be found in the bibliography of this paper). A micromechanical description of granular material behavior such as described in this paper is important if one is to have a thorough understanding of the macromechanical response of particulate media. However, some important notes must be made to clarify the main points addressed in this paper.

The linear-spring assumption representing the particle-to-particle interaction at points of contact results in an elastic instantaneous macroscopic soil response. The use of Hill's self-consistent formulation leads to an overall moduli tensor that is generally asymmetric, as correctly pointed out in this paper. However, in general there exists another type of macroscopic deformation associated with kinematic sliding between particles at original points of contact. The kinematics of this type of deformation is similar to the gliding of crystals over crystallographic slip planes in metal plasticity, which gives rise to irrecoverable deformation. Plastic deformation generally dominates the behavior of all soils, including cemented sands, and hence, without this feature the micromechanical representation described in this paper may be realistic only at very low strain levels.

The linear-spring assumption between the contacting soil particles also inhibits the interpretation of the influence of strain rates on the macroscopic soil response. It is known that even at low strain levels a soil may exhibit viscous response. Strain-rate-dependency is specially evident in soils undergoing cyclic excitation (e.g., resonant column test). An appropriate particle-to-particle rheology capable of interpreting the macroscopic viscous soil response is perhaps provided by a dashpot connection similar to the model presented by Kuhn (1987).

Finally, it must be emphasized that although a micromechanical theory may be useful for interpreting the macroscopic soil response, it may have very little practical value for the analysis of typical boundary-value problems. The cost of a full micromechanical-macromechanical analysis is enormous even for the simplest cases. (Imagine the cost associated with tracking the motion of 200 particles per Gauss point, where the continuum responses are sampled, in a finite element mesh composed of several thousand Gauss points.) The formulation presented in this paper should not be construed as a substitute for the conventional phenomenological models used for the analysis of general boundary-value problems.

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DISCUSSION by Predrag Kvasnička,
Faculty of Civil Engineering,
University of Zagreb, Yugoslavia on

"Seismic Downhole, CPT and DMT Correlations in Sand" by T.G. Thomann & Hryciw (PAPER No. 137)

The results from SCPT and DMT in-situ tests in a cohesionless soil at the University of Michigan Biological Station in Pellston, Michigan, are presented in the paper. The tests were performed with intention to compare two previously published correlations for estimating the elastic shear modulus (G^e).

The authors conclude that the most accurate and efficient method for empirically determining G^e from penetration tests appears to be one that utilizes the DMT based $_t$ and K_o.

This conclusion is based on data presented at figure 6. for two test locations 3,35 m apart. SCPT data served for precise determination of G^e at both locations.

It seems strange that both correlations give almost 100% error at shallow depths. Similar situation is at depth of 7,0 m. It's strange that G^e, according to SCPT, rises from approx. 400 bars at 4,0 m to almost 2.000 at 7,0 m. It means that shear wave velocity rises in homogeneous material from approximately 140 m/s to 320 m/s in three meters. I think that at least one more method for determining the in situ shear wave velocities such as cross-hole should be used, to confirm the SCPT data.

The conclusion that DMT based method is the most accurate and efficient (from penetration tests) is not well supported by test-site results, since the empirical total unit weights from the DMT were found to be unreasonable.

Discussion on paper titled: "Stiffness and Damping of Sands in Torsion Shear," by Supot Teachavorasinskun, Satoru Shibuya, Fumio Tatsuoka, Hiroyuki Kato, Noriyuki Horii, Paper No. 1.40
By: Anil Misra, Assistant Professor of Civil Engineering, University of Missouri, 600 W Mechanic, Independence, Missouri

The paper by Teachavorasinskun et al. will add to the database of experimental results on the initial tangent shear modulus, secant modulus and damping ratio of sands. The authors employed a torsional shear apparatus with hollow cylindrical samples to obtain the moduli and damping ratios for four sands. The advantage of the torsional shear apparatus employed by the authors is the shear strain range for which the apparatus can be used. The results obtained by the authors are mostly consistent with those obtained by the previous investigators working with other sands and apparatus.

However, some interesting results are reported by the authors on the dependency of the damping ratios on confining stress. While Hamaoka sand exhibit a decrease in the damping ratios with increasing confining stress, no such trend is displayed by the Onahama sand. Perhaps the authors should dwell upon investigating the reasons for this result, especially since the two sands have similar physical characteristics.

Furthermore, the damping ratios obtained by the authors are lower compared to those available in literature. The authors attribute this to an inaccuracy in the shear strain measurements by the other investigators. This claim, however, is not substantiated. Perhaps these differences are due to the variation in the stiffnesses of the apparatus used by the different investigators!

Discussion on paper titled: "Development of Shear Modulus Reduction Curves Based on Lotung Downhole Ground Motion Data"
by: C.-Y. Chang, C.M. Mok, M.S. Power, Y.K. Tang, H.T. Tang, and J.C. Stepp
Paper No. 1.44
Discussion by Wanda Henke, Dynamic In Situ Geotechnical Testing, Inc.

In this paper the authors discuss the development of shear modulus reduction curves using accelerograms obtained from a downhole array of triaxial accelerometers at the Lotung Experiment Site in Taiwan. The purpose of this work was to evaluate laboratory tests conducted for the site. The authors describe instrumentation, site conditions, and ground motions recorded during seven earthquakes in 1986. The analysis procedure used to estimate shear modulus as a function of strain from field data is presented. The estimates from field data are presented and compared to those from laboratory tests.

I believe this type of study is of significant value to the advancement of earthquake engineering. The broad implications of the study draw a number of comments.

Based on some of the issues discussed above, I feel it is difficult to draw solid conclusions over laboratory practices for estimating in situ shear moduli for higher levels of strain. However, I strongly agree with the authors' final conclusion that additional array data need to be collected for various site conditions. I would add that instrumenting various sites of shallow medium dense sand may also offer important benefits. In view of our current level of understanding, such sites may be among the most hazardous because they present so much uncertainty in design.

With respect to the analysis method for estimating shear modulus vs strain, the equivalent linear based approach used seems somewhat involved without an apparent advantage. For example, the approach requires laboratory data to define damping, a seemingly dominant parameter. Use of nonlinear approaches (DESRA, CHARSOIL, etc.) along with acceleration records would seem to avoid many problems. No mention is made of why the approach used was favored.

I have several comments regarding the comparisons of results from laboratory tests to shear modulus reduction curves based on analysis of earthquake accelerograms (Figure 14). First, I feel that shear modulus reduction curves such as those in Figure 14 are somewhat ineffective for presenting data for higher levels of strain. At higher strains, because of the naturally large reductions in shear modulus, the effective scale is generally quite compressed. This can mask wide differences in behavior and abnormally large scatter. As a result, I do not believe that the curves based on field data can be extrapolated beyond 0.1% strain until more earthquake data is obtained. Second, a comparison of Figures 13 and 14 suggests that the scatter in the results from laboratory tests is far greater than that in the results from field data. This implies that the scatter in the results from laboratory tests presumably conducted on undisturbed samples can be quite significant. Finally, it is not clear whether cyclic degradation was considered in the comparisons at higher strains. The accelerograms, their spectra and the inferred strain amplitude from the higher amplitude event (LSST07) suggest that cyclic degradation may have occurred during this event.

Discussion on
"Development of Shear Modulus Reduction Curves Based on Lotung Downhole Ground Motion Data: by C.Y. Chang; C.M. Mok; M.S. Power; Y.K. Tang, H.T. Tang and J.C. Stepp; Paper No. 1.44

by

W.R. Stephenson, DSIR Land Resources
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The use of strong motion earthquake records to obtain shear modulus as a function of shear strain is a technique which is very much in touch with reality. As the authors state, sample disturbance and experimental conditions may introduce large uncertainties to laboratory evaluations of shear modulus.

Although the method apparently worked well at Lotung, other sites on flexible sediments have shown characteristics that will make its success use unlikely. Such sites are those that show responses characteristic of the excitation of different members of a family of normal modes, by each earthquake. For example, Stephenson (1974) cites the Hutt Valley, New Zealand, showing that there is no monotonic relation between strain and observed resonant frequency for a large range of ground motion.

The possibility of excitation of such whole basin modes raises a vital imperative. The method as described assigns shear strain on the basis of observe surface motion, but in a whole basin mode the amplitude of resonant motion will vary across the ground surface according to the mode shape. A knowledge of the mode shape,and of where the observation point lies on that shape, become necessary before an accurate shear strain can be defined for the whole volume of sediment.

Whole basin modes have been studied by, for example, Jiang and Kuribayashi (1988). The predicted (and observed) resonant response in such cases results in a very narrow spectral peak. Lowering and broadening of such peaks may be just as important a non-linear effect as shear modulus degradation int hat it would limit resonant amplification.

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Jiang, T. and E. Kuribayashi (1988) "The Three-Dimensional Resonance of Axisymmetric Sediment Filled Valleys", Soils and Foundations, 28 (4); 130-146.

Stephenson, W.R. (1974) "An Experimental Study of Normal Modes of Vibration of Saturated Alluvium", Proceedings of the 5th Indian Symposium on Earthquake Engineering, Roorkee, November 1974, pp 119-123.

Discussion on paper titled: "Effects of Hysteretic Shape on Dynamic Response"
by: Per B. Selnes and Farrokh Nadim
Paper No. 1.46
Discussion by Wanda Henke, Dynamic In Situ Geotechnical Testing, Inc.

In this paper the authors discuss the effects, on the dynamic response of a single-degree-of-freedom system, of differently shaped force vs displacement curves. The authors discuss the numerical analysis procedure used to obtain solutions and present the results of their analytical study.

Five hysteretic shapes were studied analytically. The five curves are described as enclosing constant areas and having constant secant stiffnesses, independent of amplitude. The authors found that the shape of the force vs displacement curve can significantly affect resonance frequency, damping, amplification, and response spectra.

I feel this type of work is a step toward improving our ability to analytically predict the behavior of soil-structure-equipment systems during earthquakes. It seems that soils that are commonly of concern in seismic design can show irregular load vs deformation behavior in laboratory tests, particularly during cyclic loading when cyclic degradation occurs. I believe that modeling irregular soil behavior more closely can significantly affect the results of analytical procedures, such as site response or soil-structure interaction procedures, and thus, seismic design.

I have some specific comments on the paper. First, the authors do not clearly explain how the various force vs displacement curves were altered as the response amplitude changed. If the areas of the force vs displacement curves were maintained constant, as is implied, this would seem to lead to behavior unrepresentative of soils. Second, I am not familiar with soils that behave as models D and E. It would be helpful for the authors to comment on this.

Discussion on: "IN SITU TORSIONAL CYLINDRICAL SHEAR TEST-LABORATORY RESULTS" by: W. Henke, R. Henke, Dynamic In Situ Geotechnical Testing, paper n° 1.48

by: T.Doanh, Geomaterials Laboratory, Ecole Nationale des Travaux Publics de l'Etat, Vaulx en Velin, France.

The prototype presented in the paper is an advance to the literature on the in-situ geotechnical testing system. Some important aspects of cyclic shear stress-strain characteristics are reported: initial liquefaction, cyclic degradation ... There is no doubt that this in-situ shear test will be further analyzed and developed in other papers. We would like to request additional technical informations on testing procedure, and make some observations.

Some differences remain between this in-situ test and the laboratory hollow cylinder apparatus:

1/ The stress state of the hollow cylinder specimen can be represented in the three-dimensional axes, (τ, p', σ_a-σ_θ), and starting from a isotropic stress state, only the linear cyclic stress paths with fixed principal stress directions can liquefy the specimen. Therefore, the proposed cyclic shear test follows an pure torsional shear stress path, and it is necessary to keep the axial stress, or the ratio of the two stresses constant. To simulated the laboratory cyclic undrained shear test, it is suggested to combine the constant volume mode and the constant pressure mode.

2/ Only the applied torque and the angular displacement were measured. This τ-γ relationship can be used to calculated the shear modulus, but the stress and strain states are not completely determined. Particularly, information concerning the pore-pressure in in-situ deposits is needed to define the initial liquefaction in a truly undrained conditions. Attempt has been made for many years to develop a satisfactory measurement of the in-situ pore-pressure, but this information is very scarce; and laboratory test data provides reliable means to quantify the behavior of the pore-pressure during cyclic loadings.

With further comprehensive testing program, this in-situ torsional shear test is a promising apparatus to estimate some characteristics of cohesionless soils.

Discussion on paper titled: "Electrical Conductivity for Evaluating Fabric and Mechanical Behavior of Granular Soils," by Lien Kwei Chien and J.C. Li, Paper No. 1.49
By: Anil Misra, Assistant Professor of Civil Engineering, University of Missouri, 600 W Mechanic, Independence, Missouri

Chien and Li present a method for measuring the electrical conductivity of sand samples during its triaxial deformation. These conductivity measurements are correlated to a so called fabric measure, the formation factor. Interestingly, the initial formation factor of a sample is found to have a linear relationship with the angle of friction at failure. The variation of formation factor with the soil deformation is also investigated. The formation factor decreases with an increase of porosity. However, it is not clear as to what the formation factor represents physically. It will be of interest to investigate the relationship between the formation factor and the contact normal distribution.

Discussion on paper titled: "Cyclic Non-linear Constitutive Equations for Sands," by T. Doanh, Paper No. 1.52

By: Anil Misra, Assistant Professor of Civil Engineering, University of Missouri, 600 W Mechanic, Independence, Missouri

The author puts forth a case for a completely phenomenological non-linear incremental relationship to describe the stress-strain behavior of sands under cyclic loading. Considerable success is achieved in replicating the trends of stress-strain response for strain controlled drained cyclic triaxial tests. However, it is expected that this success was achieved at the expense of several model parameters. It would be of interest to know the total number of parameters employed in this model and their physical justification.

Discussion on paper titled: "An Estimation of Dynamic Properties of Soils from Block Vibration Tests"
by: V.D. Miglani
Paper No. 1.62
Discussion by Wanda Henke, Dynamic In Situ Geotechnical Testing, Inc.

In this paper the author discusses problems associated with block vibration tests and presents practical methods for determining dynamic coefficients when these problems occur. The author first presents the standard methods for conducting vertical and horizontal block vibration tests and for determining the coefficients of elastic uniform compression and shear from test results. The author then discusses two problems associated with block vibration tests and methods that can be used to account for these problems.

The discussion of potential problems and remedial methods should help in avoiding these problems and, when the problems do occur, accounting for them.

The first problem the author discusses is inadequate equipment. This problem limits the data that is collected in the vertical vibration test. The author suggests two methods to estimate the resonant frequency from the partial data that is obtained. Guidelines on choosing basic equipment to avoid this problem would be helpful.

Next the author discusses the problem of disturbance to the soil supporting the block. This disturbance, if not taken into account, apparently causes inadequate estimates of dynamic coefficients. The author discusses two methods for taking disturbance into account. It seems that for one project a block that was prepared was plagued by disturbance. Then, a second block was prepared without significant disturbance (repeat test). A description of the techniques used to avoid disturbance in the repeat test would be helpful, particularly if such techniques are generally applicable.

Uncertainty in dynamic coefficients determined by block vibration tests will be greater when problems are encountered. Guidelines on the levels of uncertainty that might be expected when using the proposed methods under various conditions would be helpful.

Reply to discussion by Prof. Anil Misra on paper: "Modelling the Deformation of Sand during Cyclic Rotation of Principal Stress Directions," by Marte Gutierrez, Kenji Ishihara and Ikuo Towhata
Paper No. 1.3

The authors are grateful to the discusser's comments and for bringing out an aspect of our model not hitherto discussed by the authors. Two points were raised by the discusser: (i) the experimental validity of the flow rule and the yield criteria, and (ii) the non-symmetric nature of the resulting stress-strain relation.

The validity of the flow rule is the subject of a forthcoming paper by the authors (Gutierrez et al., 1991) where the results of an extensive series of stress probe tests validating the proposed flow rule is presented. In the interesting example given by the discusser, the validity of the proposed flow rule for stress paths following the failure surface was brought up. At the outset, the authors point out that we are assuming in the model that sand flows continuously at failure, i.e., upon contact of the failure surface by the stress path, and that further loading is no longer possible. This can be gleaned by referring to Eq. (27) of the paper which gives a zero plastic hardening modulus (implying infinite plastic deformation) when the stress ratio r is equal to the stress ratio at failure r_f. By this definition, stress paths touching and later following the failure surface would not be possible.

The experimental verification of whether loading and further straining is possible along the failure surface would be extremely formidable. The test, which should be carried out in stress-controlled manner will require careful control of the stress path. Any slight divergence of the stress path outside the failure surface would be catastrophic and would mean the end of the test. The authors, however, performed a series of pure stress rotation tests at different constant angles of mobilized friction with some tests approaching and reaching the failure surface. The stress paths for these tests, which appear as circular segments, are shown in Fig. 1 together with the plastic strain increment vectors superimposed on the stress paths. Close to failure, the stress increment vectors are almost tangential to the failure surface but as can be seen, the plastic strain increment vectors are almost normal to the failure surface.

The validity of the yield criteria employed in the model is more difficult to show experimentally. There are two assumptions used in the yield criteria: i) a vanishingly small elastic region, and ii) deviatoric normality. The first assumption is based on numerous experimental results in the literature showing that loading is always accompanied by permanent deformations no matter how small the magnitude of loading. Normality in the X-Y stress space is supported by experimental results by the authors (Gutierrez, 1989) showing that the postulated loading index $d\lambda$ in Eq. (25) compares well with experimental results for different tests involving different amounts of principal stress rotation.

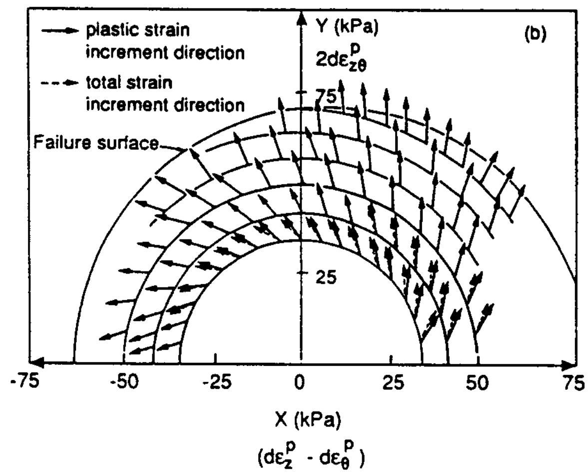

Fig. 1 Measured directions of principal plastic strain increment vectors during principal stress rotation at different constant mobilized friction angle.

On the second point raised by the discusser, we wish to point out that the non-symmetric nature of the stress-strain relation is due to the use of a non-associative flow rule in the p-q plane. The stress-dilatancy relation given in Eq. (15) leads to a plastic potential with a different form to the yield function. It is well now accepted that modelling the dilatancy of sand can only be properly accomplished via non-associative flow rules. We are, of course, in complete agreement that careful consideration with regards to stability and uniqueness of solutions should be given in the use of the model.

References

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CLOSURE on "Dynamic Properties of Saturared Coal Fly Ash" (Paper No. 1.10)

by Pei-Ji Yu and Wei-Qin Qin, IWHR, China.

As what was noted in the paper the most essential factor which causes the characters of fly ash is that it has been subjected to pulverization and high temperature during combustion. Thus fly ash is nonplasticity due to lack of clay mineral although its fines content is quite high. But meanwhile fly ash has pozzolanic action. The latter character causes the aging effect of fly ash and the former one makes the properties of fly ash different from that of natural soils with similar gradations.

The fines, which are defined as the soil grains smaller than 0.074 mm in size, include silt and clay grains. The properties of soils are greatly affected by clay content rather than by fines content. Although silt and clay are differentiated by their behavior rather than by their size the two factors are usually related to each other for a large part of natural soils. Thus in Chinese Code the clay content, P_c, which is defined as the percent, by weight, of soil grains smaller than 0.005 mm in size, is used to be a parameter to evaluate the critical STP N-values: For clay content greater than three percent the greater the clay content the higher the resistance to liquefaction; and the soil deposits of concern are considered not to be liquefied when the clay content P_c is greater than the critical value which is specified as 10%, 13% and 16% for earthquake intensity of 7, 8 and 9 degree respectively.

Townsend(1978) summarized that the cyclic strength decreases as the mean grain diameter d_{50} decreases until the value of d_{50} equals to 0.1 mm and then the cyclic sttength increases as d_{50} decreases from 0.1 to 0.01 mm. It is considered that the latter behavior is produced by the increase of the clay content in the soil concerned. For some special soils the fines of which mainly consist of silt the cyclic strength may decrease with the decrease of d_{50} to a value smaller than 0.1 mm, as what was showed in Fig. 8 in the paper for the silt taken from the lower reaches of Yellow River, reported by Jiang and Wang(1986). It is known that the main crystals of fly ash include mullite and quartz. There is no clay mineral in fly ash although its fines content is quite high. That is why the cyclic strength ratio of fly ash is smaller for greater percent of fines for a normalized relative density of Dr=50% when the aging effect is not taken into account as shown in Fig. 8.

Reply to the discussion on :
"Application of the Elastoplastic-Viscoplastic Bounding Surface Model to Cyclic Loading"
by Victor N. Kaliakin
(Paper No. 1.31)

The author would like to thank the writer for his observations, to which the following comments are addressed.

First it is important to note that *all* bounding surface formulations, whether "standard" or not, are based on the premise of inelastic deformations occurring within or on the bounding surface at a pace depending on the proximity of the actual stress point to a properly defined "image" point on the surface itself (see Fig. 1 of the conference paper). This feature sets bounding surface formulations apart from standard elastoplasticity and/or elastoviscoplasticity models and allows for the realistic simulation of the behavior of soils. Also, unlike some recent formulations that attempt to model the gradual inelastic response of cohesive soils by using multiple surfaces (e.g., Hsieh et al., 1990), the bounding surface approach is conceptually quite straightforward.

The writer is correct in pointing out the capabilities of the bounding surface model for realistically predicting cyclic and monotonic response of cohesive soils. The results presented in references cited in the conference paper bear this out. With regard to the accuracy of the model in simulating creep and stress relaxation, the author refers the writer to the paper "Verification of the Elastoplastic-Viscoplastic Bounding Surface Model for Cohesive Soils" (see list of references given below), where the simulative and predictive capabilities of the elastoplastic-viscoplastic model have been clearly shown. The results presented include drained and undrained creep, undrained stress relaxation and rate effects (with a critical discussion of the limitations of the model for predicting response at high rates).

Concerning the third paragraph of the discussion, it is important to point out that with regard to the present bounding surface model, the elastic nucleus evolves along with the bounding surface (the former is homologous to the latter). As such, there is really no "fixed" state of stress to which the solution will tend. This can better be understood by referring to Fig. 1, which shows the evolution of both the bounding surface and the elastic nucleus under conditions of undrained creep. As correctly noted by the writer, the creep process will terminate as the stress point ($\bar{I}$, $\bar{J}$) approaches (in the limit) the elastic nucleus. On the issue of continued creep of actual soils, it is well known that after the primary phase of creep, the creep rate either decreases (tending, in the limit, to zero) or, at high stress levels, increases to the tertiary phase followed by creep rupture. By controlling the size of the elastic nucleus and the specific functional form of the scalar overstress function $\bar{\phi}$ (see Eq. (5) of the conference paper), the former case can realistically be simulated using the present model (the model does not purport to simulating creep rupture well). In cases involving extremely slow, long-term creep (e.g., of embankments, etc.) the formulation may, however, require minor modification.

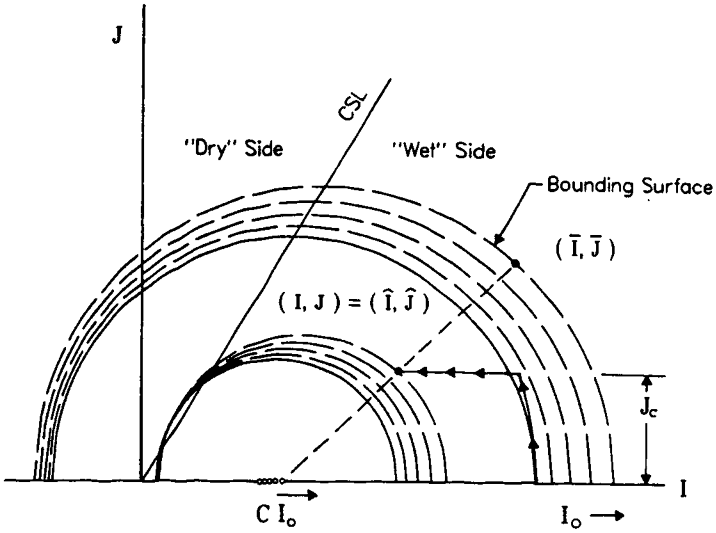

Fig.1. Evolution of the bounding surface and effective stress during undrained triaxial shearing followed by creep, terminating with state point contacting the elastic nucleus

The final topic of discussion raised by the writer, namely the question of stress point integration algorithms and return mapping strategies, is indeed an important one. The writer is correct in noting that no consistency condition is imposed within the bounding surface, and that no true yield surface exists within the surface (although the boundary of the elastic nucleus is equivalent to the concept of a yield surface, it is not identical since the stress point can cross this boundary and move outside with a smooth elastoplastic transition; i.e., no consistency condition is required for the elastic nucleus). The author would like to point out that the issues of numerical integration algorithms and radial return strategies, as applied to the bounding surface model, have been extensively studied. Since the approaches used have been described in detail elsewhere (Herrmann et al., 1987), the author will present but a brief overview of these studies.

The constitutive relations are integrated using a one-step trapezoidal rule, employing sub-stepping (if required) at the local level (i.e., at the level of the subroutines associated with the constitutive model). Except possible increased computational costs, these sub-steps are transparent to the parent program and to the user. The "robustness" of the trapezoidal integration approach has been confirmed by results presented in Herrmann et al., (1987) and by successful application of the model to actual boundary value problems (Poran et al., 1986; Shen et al., 1986; Kaliakin et al., 1990).

Regarding the question of time-stepping, it is true that typical critical state models are somewhat sensitive to the size of the solution steps. What is important to note, however, is that the bounding surface model employs the critical state concept only to relate stress and changes in void ratio and to define the "critical state" of failure. The model differs from standard critical state models in that (1) inelastic deformations can be predicted within the bounding surface; (2) any level of overconsolidation can be realistically accounted for; and, (3) time dependent response can be simulated. With regard to time-stepping sensitivity, the gradual inelastic deformation appears to lessen this phenomenon. This can, in part be attributed to the absence of an abrupt delineation between elastic and inelastic states which constitutes the essence of the bounding surface concept (in either rate-independent or rate-dependent forms of the model). Since the issue of efficient, automated time-stepping schemes is a topic of ongoing research for the author, more definitive statements cannot be made at this time.

Finally, concerning the subject of return mapping algorithms as applied to the bounding surface model, the following points are pertinent. If, in the course of an analysis, a stress point falls outside the bounding surface, a classical "radial return" scheme (Hughes, 1983) is used to bring the point back to the surface. More precisely, the point is scaled back along the line connecting the state to the projection center CI_o (see Fig.1 above or in the conference paper). This scaled stress state is then used to calculate the plastic modulus. While the return-mapping algorithm employed with the bounding surface model may not be exactly the one "used successfully in most computational plasticity problems," it affords a numerically robust constitutive model, which is what really matters after all.

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Poran, C. J., Kaliakin, V. N., Herrmann, L. R., Romstad, K. M., Lee, D.-F. and Shen, C. K., "Prediction of Trial Embankment Behavior Hertfordshire County Councils – Stansford Abbotts", Proceedings of the Reinforced Embankment on Soft Ground, King's College, London, 1986.

Shen, C. K., Sohn, J., Mish, K., Kaliakin, V. N. and Herrmann, L. R., "Centrifuge Consolidation Study for Purposes of Plasticity Theory Validation," Consolidation of Soils: Testing and Evaluation, ASTM STP 892, R. N. Yong and F. C. Townsend, Eds., 1986, 593-609.

Reply to discussions of
"Insitu Measurement of Damping of Soils" by
W.P. Stewart and R.G. Campanella
(Paper No. 1.33)

Reply to General Reporter:

The General Reporter pointed out that "A problem with this approach is to adequately account for the geometric (non-material) damping." The geometric corrections - spherical spreading, transmission-reflection, and divergence - were discussed in some detail. Provided that the shear wave velocity, $v_s \neq v_s(f)$ - see Fig. 8, paper 1.32 - then all of the geometric corrections are independent of frequency. In the recommended spectral slope method, these terms are removed by differentiation with respect to f, and therefore the geometric correction is not of concern.

Reply to W. Henke:

(1) Damping profile for spectral slope method - the method of presentation used followed the approach of Redpath et al (1982). The damping profile is in effect given by the slopes of each metre section. A type of plot similar to that used for the other methods is given in Fig. 1. As thought by Henke, this plot does show less scatter and positive damping in the clayey silt.

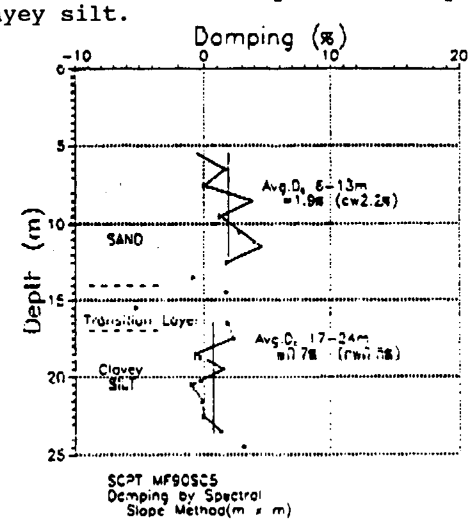

Fig. 1 Results of Fig. 13 replotted as damping profile.

(2) Repeatability of source - we have found that the swing hammer falling from a fixed height is a very repeatable source. A typical example of 4 repeated hits (used to compute coherence) is shown in Fig. 2. For the two signals with the greatest difference in the maximum value, the cross-correlation coefficient is 0.999723, indicating a highly repeatable source.

(3) Soil-probe interaction - although this is always of concern, we have found no indication that such interaction is occurring in a manner that affects the calculation of velocity or damping. In one experiment we used two cones. One was left at a depth of 3.9m as a reference. The truck was driven forward 0.25m and a second cone was pushed to a depth of 24.9m. The same damping results were obtained using the reference cone or the moving cone record at 3.9m. These results also confirmed the repeatability of the trigger and the energy source.

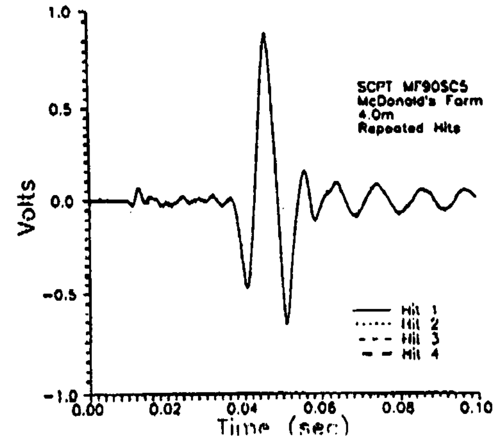

Fig. 2 Repeatability of swing hammer source.

It is felt that the measured variations in damping are at least partly caused by the natural variations in the soil. A cone bearing log for the profile used in the paper is given in Fig. 3. Although a single layer of constant damping value has been assumed over a depth of 6 to 13m, it is clear that there are significant variations in density, as in other properties as well. The values being measured are very small, especially in the clayey silt, and therefore at the present time the most useful treatment would appear to be the calculation of an average damping value over a layer of about a few metres in thickness.

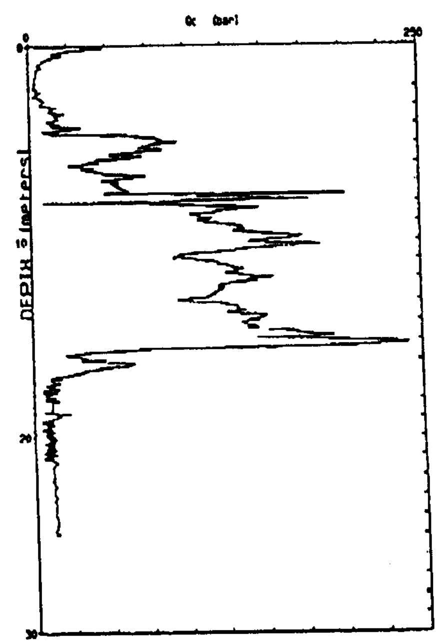

Fig. 3 Cone bearing profile of SCPT MF90SC5.

Reply to the discussion by Ronald Borja
on Paper No. 1.34 "Mechanical Properties of Cemented Sands Based on Inter-Particle Contact Behavior" by Anil Misra and Ching Chang

Reply to Ronaldo I. Borja:

The authors appreciate the points raised in the discussion presented by Borja. The main point addressed in the discussion is regarding the representation of the particle-to-particle interaction. The authors agree that to model plastic and time dependent behavior of soil the inter-particle interaction must account for sliding and time effects at a contact. However, the intent of the present paper was to model the elastic, albeit non-linear, behavior of cemented sands only. In this respect, a non-linear force-dependent spring model was assumed to represent the inter-particle interaction (see Eq. 4 and 6 of the paper). Efforts to model elasto-plastic behavior of granular media including the effect of particle sliding have been presented elsewhere (see for example Misra 1991).

Reference:

Misra, A., "Constitutive relationships for granular solids with particle slidings and fabric changes," Ph.D. dissertation, University of Massachusetts, Amherst, 1991.

Paper No. 1.40

Reply to the discussion by Assit. Prof. Misra Anil, University of Missouri

On : Stiffness and Damping of sand in torsion shear

By : Teachavorasinskun Supot, Satoru Shibuya, Fumio Tatsuoka, Hiroyuki Kato and Noriyuki Horii

It is our pleasure to have such a nice discussion from Prof. Anil Misra. The discussion is focused on the parameters affecting the damping ratio of sands; i.e., pressure level and apparatus.

Pressure level dependency : The discusser pointed out that the damping of Onahama sand looks independent on the pressure level, which contradicts to the result obtained for Hamaoka sand. In order to answer this question, Fig.(a1) and Fig.(a2) were prepared, in which the relationships between the normalized shear modulus and the shear strain of Hamaoka and Onahama are respectively shown. It is clear that the mean effective stress has an apparently different influence on the measured moduli of these two materials. Namely, a larger degree of pressure-level dependency could be observed for Hamaoka sand. Since the relationships between the normalized shear moduli and damping ratio are rather free from the effect of mean effective stress (Fig.13), these two kinds of results imply that the the dependency of damping, as well as shear modulus, on pressure level of Onahama sand is much smaller than that of Hamaoka sand. It should be noted that, for Onahama sand, tests were carried out on very loose specimens, while relatively dense samples were prepared for Hamaoka sand. This may contribute, somehow, to the different behaviors observed between these two materials.

Apparatus dependency : The discusser also pointed out also that the values of damping observed in this study are lower, comparing to those found in the literature, which might due mostly to the differences in the stiffness of the apparatuses. Referring to Fig.14, in general, the following possible experimental deficiencies may affect the measured damping values, and the magnitudes of these errors may be different among different apparatuses. 1) When the shear stress is measured outside the triaxial cell, the effect of the piston friction cannot be neglected (in our system, the shear stress was measured inside the triaxial cell). 2) The effect of the hysteresis of the displacement transducer (in our system, no such effect is involved). 3) Error due to the time lag between the recorded shear stress and shear strain. The effect is larger as the loading frequency increases. On the other hand, this error can be ignored when test is performed at a very slow frequency. This is the case for Curve No.9 shown in Fig.14. Since the tendencies of other curves obtained from tests performing at larger loading rates are similar to that of curve No.9, it is considered that the effect of this factor on the other data is also small. 4) The effect of the wave shape of cyclic stress; i.e., if, in one cycle, the stress rate is not constant and it is particularly slow near and at the applied peak stress, creep deformation could be taken place which would result in the larger values of damping. In our tests, the sinousoidal wave form has been used. The study into the effects of those factors are being underway in the authors' laboratory and the results will be reported elsewhere soon by the authors.

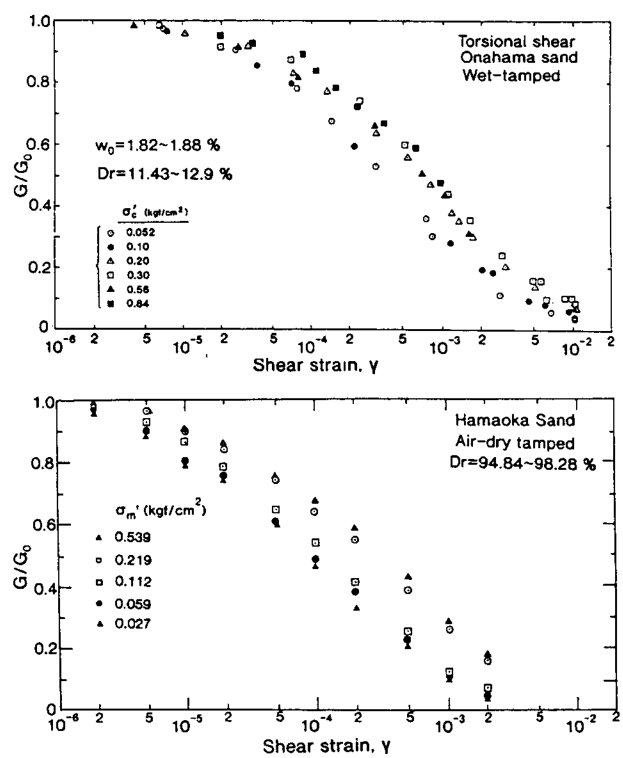

Fig.a(1) and a(2): The relationships between the normalized shear modulus and shear strain of Onahama and Hamaoka sand

Reply to Discussions on Paper No. 1.44 "Development of Shear Modulus Reduction Curves Based on Lotung Downhole Ground Motion Data" by C.-Y. Chang, C.M. Mok, M.S. Power; Y.K. Tang, H.T. Tang, and J.C. Stepp

Reply to Wanda Henke:

The authors are thankful to Wanda Henke for her interest in the paper.

It is true that different material models can be calibrated by using downhole acceleration records to obtain sets of "best-fit" parameters. The merit of the equivalent linear based approach for system identification is the explicit analytical results. Shear wave velocity and equivalent damping ratio can be determined explicitly from the location and amplitude of peak amplification without the need to formulate an optimization objective function which can be quite arbitrary and subjective. Once the shear wave velocity and equivalent damping ratio profile has been identified, shear strain can also be determined explicitly from wave propagation theory. Nevertheless, because of the presence of noise and nonlinear material behavior, the ratios of Fourier coefficients thus obtained are far from smooth. Ordinary numerical smoothening techniques using frequency windows significantly affect the amplitude of Fourier Ratios. Only the frequency of peak amplification can be determined with reasonable confidence. The large spacing between accelerometers at different depths, in addition to the difficulties in getting accurate displacement time histories from acceleration recordings, makes it difficult to calculate with confidence the strain directly from the recorded data. Therefore, strain is calculated using the equivalent linear procedure with laboratory-determined equivalent damping ratio curves. Our sensitivity studies indicated that the backcalculated values of strain are insensitive to a reasonable range of damping ratio curves.

The authors agree that shear modulus reduction curves cannot be extrapolated with confidence to a high strain range unless further earthquake data is obtained. The authors did not intend to extrapolate the curves to a strain level higher than that developed during the events under investigation.

Cyclic degradation was not considered mainly because the motions of most events are dominated by only a few cycles of large level shaking. For example, event LSST07, as cited by Wanda Henke, has a single cycle of strong pulse occurring at about 10 seconds after the recording instrument is triggered. Although cyclic degradation affects the material behavior after the strong pulse, it is believed that its effects on the response is insignificant.

Reply to W.R. Stephenson:

The authors would like to thank W.R. Stephenson for his comments.

The authors agree that whole basin vibration modes are important in the aspect of how a soil deposit in a small curvature basin responds to dynamic excitation. However, the Lotung site is not located at the edge of a basin and the curvature of Lotung basin is too flat to significantly affect the wave amplification pattern within the soil deposit at shallow depth. The ratios of Fourier coefficients are computed using recordings at shallow depths; recordings are at surface, 6m, 11m, 17m and 47m deep while the scale of the basin is in the order of kilometers.

W.R. Stephenson (1974) cited Hutt Valley, New Zealand as an example that there is no monotonic relation between strain and observed resonant frequency for a 'large' range of ground motion. The authors noted that 10 out of the 11 motions studies had peak accelerations of less than 0.03 g; these are too small for any significant nonlinear effects to be observed. The terms 'natural frequency and resonant frequency' are believed to have been mistakenly used to represent the frequency corresponding to the peak in the power spectrum. This frequency is more appropriately named 'predominant frequency' which is a combined result of soil system behavior and input motion characteristics.

Reference:

Stephenson, W.R. (1974) "An Experimental Study of Normal Modes of Vibration of Saturated Alluvium", Proceedings of the 5th Indian Symposium on Earthquake Engineering, Rorkee, November 1974, pp 119-123

Authors reply to paper titled:

EFFECTS OF HYSTERETIC SHAPE ON DYNAMIC RESPONSE

by P.B. Selnes and F. Nadim, Paper No. 1.46

The authors wish to thank Wanda Henke for the discussion. We agree that a constant area of the force vs. displacement curve independent of the level of amplitude is not representative of soils. The main objective of this paper was, however, to study possible effects of the hysteretic shape alone; a next step in the research could be to use a more realistic model with other non-linear effects included to see if this would change the hysteretic shape effects. We do not however, expect that the use of a more realistic model would change the main conclusion of the paper: that a highly non-linear hysteretic shape may have a significant effect on the reasonance period and the spectral response for dynamic loading.

Shape D is from a cyclic load controlled triaxial test on a dense North Sea sand with non-symmetrical cyclic loading (i.e. the cyclic loading is symmetrical around an average shear stress greater than zero). Shape E is a mirror image of shape D and probably not representative of any soil.

Reply to Discussion by Peter M. Byrne and Li Yan (General Reporters for Session I) of Paper No. 1.48
by W. Henke and R. Henke

We wish to thank Drs. Byrne and Yan for their efforts in discussing our paper. They raised three questions about the proposed in situ testing procedure. These concern comparisons with results from existing testing methods, the possibility of disturbances, and the nonuniformity of the applied stresses. The questions are addressed below. We are at an early stage in the development of the proposed testing procedure and therefore are able to respond only in a limited manner. Each of these issues is to be addressed thoroughly as development proceeds.

Disturbances/Comparisons--We feel that the results presented in the paper suggest qualitatively that the test soil may not have been excessively disturbed, either consistently or irregularly. Basically, the test sand behaved as expected. When testing the sand in a medium dense state (formal three test series, p. 133), we observed gradual increases in the amplitudes of cyclic rotations to very high levels. When testing the sand in a denser state (practice test, p. 135), we observed a very gradual increase in the amplitude of the cyclic rotation to a moderate, limited value. When testing the sand in what we believe to be a looser state (practice test, p. 135), we observed an abrupt increase in the amplitude of the cyclic rotation. Additionally, the test results of the formal three test series were qualitatively more consistent with each other than would be expected if irregular disturbances were a strong factor. For example, it may be seen that the increases in the amplitudes of rotations (Figure 5, paper, p. 134) are quite consistent among the tests.

We feel that preliminary quantitative results (Henke and Henke, 1990), not presented in the paper, also suggest that the test soil may not have been excessively disturbed. Figure A shows, superimposed on published initial liquefaction curves (Committee on Earthquake Engineering, et al., 1985), a roughly equivalent curve for our formal three test series. Our curve shows consistency with the corresponding published curve, though somewhat greater resistance to initial liquefaction. The main reasons for the difference are likely the approximate nature of our estimates of equivalent shear stress ratio (amplitude of cyclic shear stress/initial effective vertical stress) and differences in soil conditions. Maximum peak shear strains were roughly inferred from results of the test of mid-level excitation for the first cycle ($\gamma \approx 0.2\%$) and for the cycle of initial liquefaction ($\gamma \approx 12.5\%$) and are consistent with peak shear strains inferred from corresponding published results (Committee on Earthquake Engineering, et al., 1985). The general consistency of the results from the formal three test series suggests high repeatability.

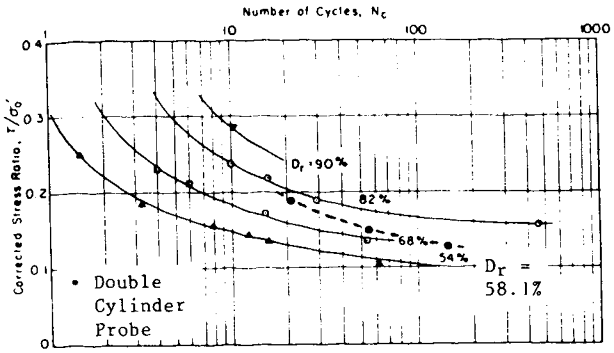

FIGURE 2-29 Stress ratio τ/σ'_o versus number of cycles to initial liquefaction, from tests on a shaking table. Source: DeAlba et al. (1976).

Figure A: Approximate Interpretations of Results from Cyclic Tests Conducted Using Double Cylinder Probe Superimposed on Results Presented by Committee on Earthquake Engineering, et al. (1985) from Large Scale Laboratory Cyclic Simple Shear Tests Conducted on Sands

Largely, similar qualitative and quantitative characteristics were observed in the only other formal three test series conducted (Henke and Henke, 1990). This cyclic test series, not presented in the paper, was conducted using a single cylinder version of the probe. It should be noted that we conducted formal tests only after conducting several practice tests to establish optimum equipment and procedures. Within each of the two formal test series, each test was conducted consistently, using optimum equipment and procedures, to provide consistent results. Only three tests were conducted in each of the series.

The quantitative results from the two formal test series do not seem to show the scatter caused by irregular disturbances that might be expected when cyclically testing recovered samples of sand. For example, the data of Marcuson and Franklin (1979) presented in Figure B below suggest that a sample having an in place medium dense relative density of about 60% (relative density for three test series presented in paper averaged 58.1%) may, on recovery, show a relative density that might range from 50% to 85% even when applying refined sampling practices. The corresponding range of cyclic characteristics would be expected to be great - readily liquefiable to highly resistant to liquefaction.

We feel that disturbances, both consistent and irregular, were largely avoided because of the many steps taken to reduce disturbances - using a casing, trimming the test soil, penetrating under pressure, using precisely machined cylinders, using a shield, and testing in place.

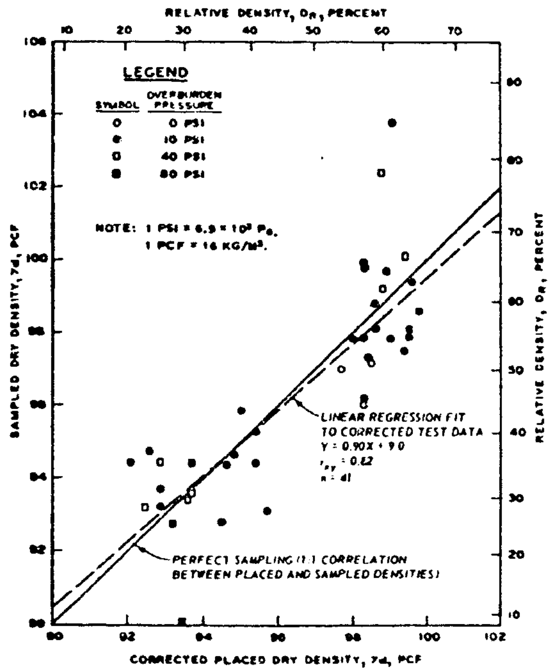

Figure B: Sampling Effects on Relative Density of Sand (from Marcuson and Franklin, 1979)

Nonuniformities--The test soil does develop, during testing, a stress field that is nonuniform in the radial and vertical directions. Nonuniformities are taken into account by simulating tests analytically to interpret results from tests (Henke and Henke, 1990). Soil characteristics are iteratively varied in the analyses until measured and computed results agree acceptably. The analyses describe nonuniformities.

Ultimately, the analyses are to describe all important aspects of tests including full three-dimensional axisymmetric torsional behavior. Thus, nonuniformities are expected to be taken fully into account. At this time, our analyses are somewhat simplified. Only nonuniformities in the radial direction are described. Behavior in the vertical direction is constrained to be uniform. This modeling is expected to be reasonably effective for preliminary estimates because we feel that radial nonuniformities are the most significant.

ADDITIONAL REFERENCE

Marcuson, W.F., III and Franklin, A.G. (1979), "State of the Art of Undisturbed Sampling of Cohesionless Soils," Miscellaneous Paper GL-79-16, Geotechnical Laboratory, U.S. Army Engineer Waterways Experiment Station, Vicksburg, Miss., July.

Reply to Discussion by T. Doanh of Paper No. 1.48
by W. Henke and R. Henke

We appreciate Dr. Doanh's suggestions and efforts in discussing our paper. Dr. Doanh pointed out two differences between the proposed in situ test and the laboratory cyclic shear test of hollow cylindrical samples. These two differences concern the character of the applied stresses and the measurements made. These issues are addressed below. First, it is important to state our objective during the present early stage of developing the proposed testing procedure. We have mainly sought to develop a procedure that applies earthquake-like cyclic shearing stresses in situ and provides detailed information, yet, is as operationally simple as possible. We considered the laboratory cyclic torsional shear testing of hollow cylindrical samples to be the ideal toward which to aim with the proposed in situ test. We compromised when necessary to maintain simplicity. We are continually evaluating compromises and planning improvements.

Applied Stresses--We agree that the laboratory cyclic torsional shear test applies stresses that are likely more representative of stresses induced during most earthquakes than those applied by the proposed in situ test. We feel, however, that for its early stage of development, this test provides a reasonable balance among inducing earthquake-like cyclic shear loads, providing detailed information, and maintaining operational simplicity. Results presented in the discussed paper, in our reply to the discussion of the paper by Drs. Byrne and Yan, and by Henke and Henke (1990) suggest that cyclic loading in the constant volume mode leads to behavior that reasonably resembles the behavior induced by various laboratory cyclic shear tests. Additionally, the test is not particularly difficult to conduct. We are continuing to evaluate the proposed procedure and plan to advance our ability to effectively apply representative loads in situ through the further development of equipment and procedures, perhaps along the lines suggested by Dr. Doanh.

Measurements--It is true that measurements of porewater pressures under reasonably undrained conditions (or effective stresses in the constant volume mode under drained or dry conditions) would provide greater insight into behavior and improve our ability to interpret results from tests. We did not initially provide this capability because it seemed possible to provide adequate information for evaluating promise without such measurements. For example, the torque and rotation measurements provide considerable information on cyclic deformation characteristics, which are of greatest interest in earthquake resistant design. Additionally, we feel these measurements can provide useful indicators of effective stress (or porewater pressure) at various stages of cyclic loading. For example, effective stresses may be considered to be roughly zero when the measured torsional stiffness (slope of the torque vs rotation curve) is roughly zero. We have considered the first cycle in which this occurs to be the cycle of initial liquefaction. We show, in our reply to the discussion of our paper by Drs. Byrne and Yan, comparisons to published results that suggest that this may be a reasonable interpretation of results. We do plan to refine the testing system to measure porewater pressure (or effective stress). We feel this will be possible and should improve our understanding of and ability to interpret results from tests.

V.D. MIGLANI, Professor of Civil Engineering, Regional Engineering College, Kurukshetra - 132119, Haryana, INDIA.

Paper title : An Estimation of Dynamic Properties of Soils from Block Vibration Tests.

Paper No. : 1.62

Author's Reply:

I would like to thank WANDA HENKE for taking keen interest in the paper and for making suggestions.

Firstly the discussor has desired guidelines on choosing basic equipment to avoid possible first problem discussed in the paper, i.e., clear resonant peak may not be available in vertical vibration tes. It may be again pointed out that occurence of such possibility is rare when equipment as per the Indian Standard Method (reference given in the paper) is used. The causes for inadequacy of the equipment (in these rare cases) are given in the paper from which it follows that in order to avoid occurence of the problem, the D.C. motor, speed control unit and their accessories must have adequately higher power rating and also higher frequency (or revolutions/sec.) range which makes the whole equipment not only cost prohibitive but also bulky. Addition and use of phase meter and/or ammeter in the equipment on the other hand is rather a very convenient alternative. Moreover, the results of 'C_u' can be adequately checked from results of 'C_t' from horizontal vibration test In my judgement, using a 5 HP rating for motor and speed control unit in place of 3 HP rating and increasing the rating of LAZEN oscillator to 60 cycles/sec by decreasing the size of its pulley may sufficiently reduce the possibility of partial peak, but may not altogether eliminate it.

No technique to avoid disturbance present under the blocks can be prescribed. It is not correct to imagine that a second block was prepared. The repeat test was conducted on the same block after a lapse of about four months when favourable conditions were existing. More details regarding source of disturbance and favourable conditions are given in the present paper and elsewhere. (MIGLANI -- 1988 a Reference given in the paper).

Level of uncertainty in dynamic coefficients that might be expected when using the proposed methods in the event of occurence of first problem is negligible for all practical purposes However should the second problem occur, the error level is comparatively higher but reasonably good estimate of dynamic coefficients can be made by using the proposed methods. Only one repeat test as reported in the paper, could be conducted and hence generalised conclusions cannot be drawn for levels of accuracy or otherwise An idea of uncertainty level can be had from the comparison of uncorrected corrected values and repeat test values given in TABLE-2 of the paper.

Reply to Discussion of "Downhole Seismic Cone Analysis using Digital Signal Processing" by R.G. Campanella and W.P. Stewart (Paper No. 1.32)

Reply to W. Henke:

(1)We have used a variety of sources - buffalo gun, P-plate, blasting caps, as well as various sizes of hammers. Due to space limitations, we discussed only the best shear source, a horizontal hammer blow on a loaded beam. We hope to discuss our experiences in more detail in a future paper.

(2) Frequencies of waves - for the steel hammer on steel beam source, we have found that the bulk of the FFT energy is centred at about 75Hz for all of the soils tested - clay,silt and sand. The effect of soil type seems to be on the shape of the FFT. Fig.9a shows a clayey silt with a fairly smooth FFT. Fig.9b shows a sand with a more irregular FFT. This result might be expected from the more irregular cone profile for the sand.

(3)Amplitudes of shear waves - are mainly determined by the sensitivity of the receiver and the noise in the system. Provided the receiver has high sensitivity and the noise is low, it is possible to amplify the signal to the desired amplitude.